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able through its connection with the sensory branch at the same time that it is not dependent upon the sensory reports of the latter for its innervation: its connections with other sensory functions may still be sufficient. But in another case (Bernard's) it may be both excitable through and dependent upon the sensory reports.

It is evident that there is another possible explanation, i. e., that while sensory reports are sufficient for the regulation of involuntary movements, they are not sufficient for voluntary movement; these latter are regulated from a higher co-ordinating centre. This alternative Chauveau suggests and rejects, although he admits the physiological difference it requires, i. e., "one (muscle) is automatic, not influenced by the psycho-physiological centres, the other is a part of the voluntary locomotor system, influenced by these centres." The difficulty with his explanation is three-fold: (a) If the dependence of the motor function in question upon the support of the sensory branch be inversely as the degree of implication of other sensory connections, then the most complex and co-ordinated function, i. e., walking, would be most free from impairment when certain of these sensory connections are cut. Such relatively simple function as moving the head up and down upon the neck—due to the contraction of the sternomastoid—we would expect to be most impaired by cutting the sensory branch. (b) We may ask why such sensory support from other connections does not also avail to prevent impaired function in the case of the involuntary muscles? (c) Even though the kinæsthetic regulation by groups of sensory nerves be allowed, we would expect some impairment of function when the entire sensory contribution from the muscle in question is cut off; the amount of laming would be a matter of degree. But this is contradicted both by the result on the sternomastoid, and by Chauveau's experiments in cutting the four nerves supplying the toes of pigeons. He found that "complete sensory enervation of an extremity does not appreciably disturb these (the locomotor) functions," and "they roost indifferently upon the normal and upon the enervated foot." I may add, also, that these experiments upon the sternomastoid are valuable for the discussion of the regulation of voluntary movements, since all kinæsthetic support from "remote" sources (the eye, ear, etc.) are here ruled out.

Other interesting results are: Contractions of facial and lingual muscles by stimulating the nuclei, in the fourth ventricle, of the seventh and twelfth pairs; contraction of abdominal and spinal muscles by stimulating sensory cells of cord between the last dorsal and first lumbar root, after section of the cord from the brain (artificial respiration being carried on); co-ordinate respiratory movements in horses, after separation of the medulla from the encephalon, by stimulating the intercostal nerves. As to the mechanism of the sensori-motor circuit thus demonstrated, Chauveau criticises the ordinary kinæsthetic theory as being too complex, and adopts the view that there is a repulsive wave which runs toward the muscle along the sensory course, reaching the muscle simultaneously with the motor impulse, and regulating the muscular contraction.

WALLER, *The Sense of Effort; An Objective Study; Brain*. LIV. and LV., 1891, p. 179 ff.

Dr. Waller attempts to ascertain by experiment the locus of the sensation of fatigue after muscular work and thereby also the locus of the sensation of muscular effort; holding that the former is related to the latter as an after-effect to a first-effect, analogous to

the relation between retinal images and after-images in vision. The main results on fatigue are as follows (he gives details of experimentation, apparatus, graphic records, etc., for which the article must be consulted): (1) The nerve courses do not contribute to fatigue. (2) There is no diminution in the lateral enlargement of a muscle from direct (artificial) stimulation after a series of maximum voluntary contractions, i. e., voluntary fatigue has not exhausted the muscle. (3) If a maximum series of voluntary effects be interpolated between two maximum series of direct effects, the second series of direct effects suffers no diminution, i. e., there has been recovery in the muscle for direct work during the series of voluntary effects. (4) The same recovery of the muscle for voluntary work during direct stimulation is seen by interpolating a series of direct effects between two series of voluntary effects. Waller agrees with Mosso, who obtained similar results, in holding that this recovery can only be in the peripheral organ in the former case and in the central organ in the latter case. (5) There is no diminution in voluntary effects after a maximum series of direct effects. (6) In the longitudinal effect there is a diminution both in direct maxima during voluntary work, and (less) in voluntary maxima during direct stimulation: a result which again agrees with that of Mosso. The general conclusion is that "voluntary fatigue depends more upon central than upon peripheral change." Further, the lateral (rigor) effect, in voluntary contraction of fatigued muscles slightly precedes and outlasts the longitudinal effect; this delay in the subsidence of the lateral effect is mainly due to a continued action of the centre upon the fatigued muscle, "a contraction reminder," "a residual discharge from the fatigued centre." Further, the cessation of voluntary longitudinal effect is quicker in fresh than in fatigued muscle. These results follow the use of a spring dynamometer rather than the weight-lifting method of Mosso. Waller criticises the latter on the ground that a weight is often lifted by more than the minimal effort really necessary to lift it. This criticism is proved by the fact that the weight method applied to the last mentioned experiment gave a longer persistence of the longitudinal than of the lateral effect, showing that some strain was relieved before the longitudinal effect actually produced by the weight was touched. The same criticism is proved to hold also *mutatis verbis* for the beginning of a weight-experiment, i. e., some force is spent before the weight begins to rise.

Waller also experiments on the flying-up of the arm when suddenly released from a voluntarily-lifted weight. He finds (using Douders's apparatus) that the upward spring of a fatigued is greater than that of a fresh arm. This agrees with Mosso's result that in a succession of such "releases" the amount of spring progressively increases. Waller explains the fact by the result given above that there is a "contraction reminder," which gives delay in the cessation of the nervous discharge in consequence of central fatigue. (This accounts both for the fact of such an upward spring and for the fact that it increases with fatigue; while Mosso's explanation, i. e., that greater stimulus is sent to the muscle in consequence of its fatigue, only accounts for the second fact.) A series of experiments is reported, further, on the estimation of weights when the muscles of the hand are "directly" stimulated. He found, in opposition to Ferrier, Goldscheider and Bernhardt (who, however, Waller claims has been misquoted in this connection), that under cathartic excitation of the digital flexors by the median nerve, 1000 gr. was barely distinguished from 3000 gr., or 500 gr. from 1500

gr., while, when voluntarily lifted, 1000 gr. and 1500 gr. were distinguished. In the case of faradic stimulation, practically no discrimination of weights was possible. This removes one of the strongest experimental arguments for the exclusively peripheral estimation of weights.

Ingenious as Waller's main argument is, it leaves two loop-holes of escape from his conclusion: (1) Granted that in fatigue the centre is mainly exhausted, it does not follow of necessity that this centre is the point of "incidence" of the feeling of fatigue. Mosso shows that central exhaustion has a chemical peripheral effect; this may occasion the feeling of fatigue. Or central expenditure may draw a kinæsthetic centre apart from itself, this latter contributing the feeling of fatigue. Waller's inference is a highly probable one, but not the only possible one. Further granted for the moment that the inference is correct, it only puts the sense of fatigue somewhere in the centre, not necessarily in the motor seat, unless we agree with Waller in denying the distinction between sensor and motor rests. (2) Granting the full value of the experiments, they again go no farther than to render probable the point of "incidence" of the sense of effort. Such a feeling may still arise in a kinæsthetic centre in dynamic connection with the working motor centre, or from "remote" peripheral courses. Nevertheless, logical alternatives aside, Waller's conclusion is undoubtedly the best interpretation of Waller's premises.

DELABARRE, *The Influence of Muscular States on Consciousness; Mind.* N. S. 3, July, '92.

This paper is largely a summary of the author's *Ueber Bewegungsempfindungen*, which will be noticed later. In this connection, only Delabarre's criticism of Waller may be referred to. Delabarre claims that Waller's argument contains three assumptions: (1) "That the objective signs of exhaustion are always indicative of a previous expenditure of energy in the same parts." [Not so. Waller uses the same muscle for voluntary and direct excitation. His assumption is: Granted both voluntary and direct work from the same muscle and objective exhaustion only for voluntary work, then the objective exhaustion must be in the nervous centre for this muscle. This is valid, and Delabarre's statement is a fallacy of conversion.] (2) "That a subjective sense of fatigue is indicative of a corresponding previous effort, and sense of effort, in the same parts." [Not so. Another involved conversion. Waller's position is: Granted a maximum voluntary effort and sense of effort directed to a part, and a sense of fatigue following the exercise of the same part, then the seat of this sense of exhaustion is the same as the seat of the sense of effort. This does not necessarily follow, as I have said above, but is physiologically extremely probable.] (3) "That objective signs of exhaustion are indicative of a subjective sense of fatigue, and objective signs of effort of a subjective sense of effort, localized in the same parts." [I also find this assumption, and have above expressed an opinion of the extent to which it invalidates Waller's conclusion.]

MÜNSTERBERG, *Mitbewegungen, in Beiträge zur experimentellen Psychologie*, Heft IV., 192.

Professor Münsterberg gives a résumé of the discussion on the question of the concurrent innervation of symmetrical movements on the two sides of the body, against the old view that there was a direct tendency to such symmetrical movements when either side